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DEPARTMENT OF BIOCHEMISTRY

May 20, 1959

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Dear Arthur,

I was pleased to get your postcard from Russia. It sounds as if you had a good trip.

Everyone here expects to leave for Stanford in time to arrive the first week in July. I'll hope to arrive just about July 1; fortunately the Colloid Symposium in Minneapolis is earlier in June than I had remembered.

My request for research funds to the National Science Foundation has been approved (without any cuts! -- but it was a modest request). And it seems probable that the N.I.H. fund for work on casein can be transferred to Stanford, although this may take some time. But if the two graduate students can be supported for a while out of the training grant, I should have sufficient funds to meet my commitments.

In regard to decisions on equipment, the spectropolarimeter has caused me the most concern. Basically the problem is this. At present there is only one instrument available (the Rudolph) which is satisfactory for studies of the rotatory dispersion of macromolecules. This is the instrument used in every major investigation in this field - for example in the current work of Doty, Schellman and Harrington. On the other hand Zeiss is bringing out an instrument which is not suitable for dispersion studies but which should be almost as good as the Rudolph for measurements at a single wave length in the visible. This instrument will be cheaper than the Rudolph and undoubtedly more rugged. We really should have an instrument for studying dispersion, however. We can do many studies at a single wave length, but I hate to be limited to this - particularly since measurements of the dispersion itself have become routine in the protein field. Now Zeiss also say (or at least their man at the Federation Meetings said) that in the future they will bring out such an instrument. Thus far the Zeiss representatives in this country (Brinkmann) won't even quote me a delivery time on the single wave length instrument.

My conclusion is this: if we are pinched for funds for equipment, we probably can squeeze by with the Zeiss single wave length instrument. If we are not pinched for funds we should order the Rudolph now: it will be about \$11,500 and would be delivered in 4 to 5 months. I will send the technical data to

** which I would guess can be obtained in six months*

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Peter Hoefer, along with some other quotations on equipment. The Zeiss instrument is not more accurate than the Rudolph (in fact, slightly less so) but it undoubtedly is better made and more reasonably priced. (I expect the single wave length instrument - for which I have not yet got a formal quotation - to be about \$4,500.) I will leave the actual decision up to you, on the basis of how far the funds for equipment are going. We certainly will need a photoelectric polarimeter of some kind within roughly six months. I believe I've canvassed the other possibilities (Applied Physics, Perkin-Elmer, Leitz, Standard Polarimeter). Leitz will probably have a satisfactory instrument, but not until a year from now at the earliest. And they wouldn't quote a price.

Finally one more thorny question, that of the Australian physical chemist interested in postdoctorate work. I'm enclosing copies of his letter to me and also my reply. I've written Professor Jordan for his estimate of Inman's abilities, but I think that probably I have a pretty good picture of these already. A good friend of mine, Peter Dunlop, is now a senior lecturer in Jordan's dept. and has written me about Inman. Also Mike Creeth, who is doing some sabbatical research in Chemistry here, was at Adelaide until a month ago and knows Inman quite well. Mike taught the physical chemistry lab, and Inman was a student "demonstrator." Both Mike and Peter agree that Inman is a clever experimentalist, a mature research worker, a pleasant person to work with, and not very mathematically inclined, as physical chemists go.

In the meantime I've discussed with Gerry Wake the possibility of doing physical studies on DNA. (Gerry is the postdoctorate fellow who has been working with me on casein and who will go along to Stanford.) He is not only willing but enthusiastic about doing this. When I wrote to him from Copenhagen about my going to Stanford, I emphasized that he would be free to continue his studies on casein if he chose to. His Ph.D. work was on casein, and he was anxious to continue with this in doing postdoctorate work. However time and distance have a way of changing one's point of view.

At any rate, the point is this. I can get work going on the enzymatically synthesized DNA whether or not Inman comes. Being somewhat theoretically inclined myself, I prefer to work with someone who can do experiments efficiently when it comes to a project where lots of experiments are in order. And Gerry certainly would fit in well here. I've been very enthusiastic about the work he's done here on casein. He's succeeded in something I've tried to do for several years, namely separate the fractions of casein by a direct method (the one which has finally worked is gel electrophoresis in concentrated urea solutions). He was trained as a physical biochemist in a biochemistry dept. (at Sydney), and knows about as much about DNA as I do. Nevertheless I think he will learn fast.

So again I will leave the decision to you. If we were to get someone trained in the physical chemistry of DNA to work on the enzymatic synthesis, I doubt if we would find someone better than Inman. On the other hand, if it seems desirable to hold down the numbers in the department, I'm confident that we could get a good program on DNA under way without him.

Sincerely yours,



Robert L. Baldwin